A Study of the Effect of Preceding Stimuli Upon the Judgment of Auditory Intensities

BY
LOUIS LONG, Ph.D.

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CHAPTER I

INTRODUCTION

A. Statement of the Problem

This is a report of a purely psychophysical investigation. It is an attempt to test the hypothesis that preceding stimuli affect the judgments of the succeeding stimuli. To be more specific, in the literature one finds evidence of various kinds that if a weak stimulus is presented after a strong one, the judgment of the second stimulus will be affected by the fact that the strong stimulus preceded. Just how it will be affected is still an unsettled question, as will be pointed out. Recent investigations seem to indicate that when a weak stimulus is presented after a strong one, the weaker is judged still weaker; this is the phenomenon that has been called the contrast effect; it has been found experimentally in various sensory fields.

The present experiment was planned for the purpose of seeing whether any such effect is present in the field of auditory intensities. If it is present, will it be a contrast effect or will the results conform to the law of central tendency; or do both operate? If such an effect exists it would be well to have an absolute measure of the magnitude of it. As yet there has been no such measure obtained, but the present experiment does involve such an approach.

The purpose of the experiment can be thus outlined in very broad and general terms. When the problem is stated more specifically one sees that it is an investigation concerned with what has always been called the time error in psychophysical experiments. In many experiments it has been found that there is an over-estimation (i.e., a negative time error) or an under-estimation (i.e., a positive time error) of the second stimulus with regard to the first. Attempts have been made to compensate for this asymmetry by reversing the order of presentation of the stimuli or by using a standard above the mid-point of the series; but in most cases the time error was not completely eliminated.

Until recently the temporal relation of the two stimuli has been considered the major factor for producing this asymmetry, probably in large part because of the methodology of presenting two successive stimuli. But it has been found (23; 31) that the same phenomenon occurs when only one stimulus at a time is presented for judgment by absolute impression (method of single stimuli). Con-

sequently, it can no longer be contended that the time error is peculiar to the methods using paired stimuli.

B. Review of the Literature

Fechner observed the time error in many of his experiments and offered theoretical interpretations of this phenomenon (8). Successive comparison, he assumed, necessitates the comparison of the present (or second) sensation with the image of the earlier (or first) stimulus. The time error could then be explained by the "fading" of the image. If the image of the first stimulus fades, its strength is decreased, thereby causing a relative over-estimation of the second stimulus. The above assumption demands that the error of over-estimation should increase as the interval between the comparison stimuli increases. Fechner verified this experimentally and as a consequence the theory was accepted for many years. But later experimental work (2; 32) proved that the image was not the basis for the comparison of the second stimulus with the first. The judgments in many cases were found to be "immediate" or direct, with a complete absence of images.

Martin and Müller in their attempt to analyze the factors involved in the making of a comparison postulated the presence of fatigue and its opposite, *Bahnung* (22). The lifting of the first weight may cause the motor centers to become fatigued, which in turn would cause the second weight to seem heavier since the energy available in the second lifting will be less than that of the first. But the opposite effect was also found; the lifting of the first weight may not produce fatigue, but rather a type of facilitation, and therefore the second weight is sensed as correspondingly lighter. These investigators also found that the actual comparisons are not always between the two weights that are given to be compared, but are frequently between one of them and the corresponding weight of the preceding pair. This has come to be known as "side comparison" (35). Finally they found that the factor of absolute impression plays a part in the judgment rendered. As the observer becomes oriented to the situation he gains a very definite impression of the stimuli and when he lifts a weight he has a feeling that it is heavy or that it is light—not as the result of a definite comparison of the weight with the standard—but rather, as a mere impression attached to the stimulus itself.

The next theory that we wish to discuss is that of the Gestalt School, which is based largely on the work of Köhler (20) and

Lauenstein (21). This theory can be reduced to two basic assumptions: (1) the gradient or stepwise phenomenon and (2) the trace hypothesis.

When two things are compared they are seen to be in some functional relationship within a whole graded system. The relative position of the objects within the system determines the comparison rendered. This gradient of potential is located between the point of excitation of the second stimulation and the trace of the succeeding excitation. That is, a stimulation influences a succeeding stimulation, not directly, but through some effect which has been left behind. This effect is what is meant by the trace of the first excitation. If two excitations occurred at the same place in the brain, then the trace of the first stimulus would be changed by excitation of the second stimulus and consequently comparison would be impossible. Thus it is essential to assume that the second excitation occurs in a different part of the brain; the "place of a process is different from the place of the traces" (19, p. 441).

This dynamic communication or interaction between the process and the trace is postulated as being due to a leap of electrical potential between the two adjacent areas, or between each of them and the field that separates them, if they do not adjoin. The two stimulations will cause chemical reactions in the corresponding adjacent areas. These reactions will produce different concentrations of reacting molecules and ions, which because of the different velocities of positive and negative ions, will result in a difference of potential. If a field of different stimulation intervenes between these two areas, then the three areas will form a unitary dynamic field, the properties of which will be determined by the relative concentration in the three areas.

The question arises as to what happens to this trace during the interval between the first and second stimulus. If you assume that the concentration of the trace decreases, then over-estimation of the second stimulus (which, for purposes of illustration, is assumed to be objectively equal to the first stimulus) would be explained, for by comparison with the trace of the first stimulus it would seem greater. But suppose you assume that the trace of the first stimulus increases in concentration, then this would explain the under-estimation of the second stimulus (which is again assumed to be equal to the first). These assumptions are in line with the observed fact that reactions caused by stimuli attain their maximum concentration not instantly, but after a brief period of increasing strength, after which the con-

centration decreases in a similar manner. When Köhler found under-estimation of the second stimulus up to a certain interval (about three seconds) between two stimuli and over-estimation beyond this interval, he concluded that the rising and falling of concentration of the first stimulus could be used to account for the time error.

For an explanation of the cause of these changes Lauenstein formulated two hypotheses: "We can either assume that all traces are gradually being destroyed by metabolic processes and that this causes the negative time error with longer intervals. Or we can assume that under the conditions of these experiments traces in proximity become assimilated to each other. In that case the negative time error would be explained by an assimilation of the trace of the first excitation to the trace of the state corresponding to lack of stimulation" (19, p. 468). These hypotheses were tested in the visual and auditory fields by varying the background. In one series the background was strong, in another, weak. The comparison stimuli were the same for both series and were between the extremes used for backgrounds. He found a preponderance of "weaker" judgments for both backgrounds with very short intervals, but as the intervals increased in length the error of estimation also increased, being an error of over-estimation with weak ground and of under-estimation with strong ground. Thus the individual trace assimilates to the trace system of its background—if the latter is weak, then the trace of the first stimulus moves towards it and consequently the succeeding stimulus is over-estimated.

The concept of assimilation of traces implies that traces which remain after stimulation are related functionally with other traces, as a result of the change they undergo. The results of Lauenstein support this assumption, as does Köhler's finding that with continued experimentation the error changes from over- to under-estimation. Thus traces produced by the same type of excitation "do not remain independent of each other but form larger trace systems which influence newly formed traces" (19, p. 473). A trace which during the first experimental sitting lost some of its concentration and thereby caused over-estimation, will as a result of other sittings increase its concentration and consequently produce under-estimation.

The theory offered by Woodrow differs from the Gestalt theory mainly in a shift of emphasis. Woodrow's theory is psychological, with little or no attempt to give it a physiological basis. In harmony with the above modification is the use of psychological, rather

than physiological, concepts. The most radical change consists in discarding the term "trace," along with all of its physiological implications, and the substitution of the term "set," by which is meant "a readiness for a stimulus of a particular character" (34, p. 408). Woodrow believes that any of the following words might be just as appropriate as the one he adopts: attitude, expectancy, adjustment, adaptation, or attunement. What word is used is merely a matter of opinion; but the point is that it should imply only "a readiness for a stimulus of a particular character" (34, p. 408).

In order to explain the facts of comparison Woodrow makes the following assumptions:

1. "In the case of the comparison of intensities, the set existing . . . is a set towards a certain intensity" (34, p. 409), which is referred to as the level.

2. "The level of the set tends to approximate the average intensity of all the preceding stimuli of the entire series given from the beginning of the experimental sitting, provided the series does not vary over too great a range" (34, p. 409). The factor of recency is implied here, since it is believed that the most recent stimuli have greater influence in determining the average established, and that the most recent has the greatest amount of influence. Furthermore, a concept resembling that of assimilation is involved because this assumption necessitates reverting to the average level after either a weak or a strong stimulus. Thus the level adopted following a weak stimulus will be lower than the average, but will gradually return to the average. Likewise a strong preceding stimulus will cause the level adopted to be higher than the average, but it will again revert towards the average level.

3. "The level of intensity towards which S (subject) is set tends in all cases to sink towards a lower level, but does not even remotely approach a level of zero as long as the set is maintained" (34, p. 409). Woodrow suggests that this postulate may be a special case of the second assumption since it is possible that there is "a chronic level of adjustment corresponding to the average intensity of habitual stimuli; in which case the tendency of the level to sink would be merely the tendency to adjust to an average habitual intensity" (34, p. 409).

Thus the above assumptions imply two different levels of reference: first, some permanent level which is an absolute indifference interval. This may be the result of past experience, as Katz and Wundt suggested; but Woodrow, while admitting the importance of

the effect of past experience, believes the absolute indifference levels are "largely determined by innate constitution, not simply of the sense-organs, but of the entire sensory-cerebral-motor mechanism" (33, p. 145). The second level of reference consists of the average magnitude of the entire experimental series. This level is established only through the repeated presentations of the stimuli composing the experimental series. Just how long this level of reference remains is still a matter of speculation, but Woodrow believes that it is, at least, carried over from one day to the next.

The value and importance of these assumptions can be demonstrated by turning to some experimental findings reported by Woodrow. In an experiment in which lengths of temporal intervals were estimated, Woodrow found that "at every sitting the errors shifted in the positive direction with prolongation of the sitting and at each successive sitting they also became more positive (or less negative) than at the preceding sitting" (33, p. 135).

Now these results can be explained by the assumptions discussed above. The absolute indifference interval was firmly established before the experiment began, but the other level of reference was built up only as a result of the experiment. Thus it is logical to expect that at first the former exerted a "more powerful gravitational influence" (33, p. 139) than the latter. But as the observer became habituated to the experimental series, the average magnitude of the series gradually became a level of reference; it is likely that this new level replaced the original level.

Gravitation towards the series average would cause the shorter variables to become longer; the longer variables, shorter; and the medium variables would remain unchanged. Thus there would be a negative time-order error for the longer variables, a positive error for the shorter variables, and no error for the medium variables. This was not, however, found at the beginning of the experiment; doubtless because the series average exercised relatively little influence. At this time the absolute indifference interval is more important in determining the direction of the error. The nature of this level of reference, since it is shorter than any of the variables, is such as to produce a negative time-order error for all variables.

Thus we find that the two levels of reference combine in some cases to cause a negative time-order error, but in other cases conflicting effects are produced. Both levels tend to produce a negative error in the case of longer variables; this reinforcement results in an extremely large error. But in the case of medium variables the

series average tends to produce no error, while the absolute indifference interval causes a negative error and the result is a negative time-order error. In the case of the shorter variables the absolute indifference interval calls for a negative error, while the series average requires a positive one. Since the effect of the former is greater at the beginning of the series, a negative time-order error results, but of much less magnitude than in the case of the longer variables. As the experiment progresses the influence of the absolute indifference interval is reduced and is replaced by the series average. This causes all variables to have a smaller negative error and the error for the shorter variables becomes positive.

Pratt was of the opinion that the hypothesis of assimilation could be tested by making the following deduction: if no intervening stimulus is presented between the two comparison stimuli, then the amount of over-estimation should be greater than when an interpolated stimulus is used (25). In one experimental situation Pratt used tones as stimuli to be compared in intensity, having three series with the same variables and standard, but in two of these series an interpolated stimulus was introduced. In the "loud" series a tone louder than any of the variables was interpolated; in the "weak" series a tone softer than any of the variables was interpolated; in the "normal" series no stimulus intervened. Under-estimation was found in the loud series and over-estimation in the weak. So far the results agree with Lauenstein's hypothesis, but the amount of over-estimation was greater for the weak series than for the normal. To check this Pratt used lifted weights, having a normal series with no interpolated stimulus and a light series in which a weight lighter than any of the variables was interpolated (25). His previous results were confirmed in that he found more over-estimation in the light series than in the normal. These results suggest that Lauenstein's hypothesis is an over-generalization, for it does not hold true when there is no intermediate stimulus; in this case the "trace merely *sinks*" (25, p. 297). Thus Pratt concluded that "*the effective comparison-value of the first member of a pair of stimuli presented for judgment with respect to intensity decreases in magnitude after the first three seconds, unless the impression from some secondary stimulus constitutes an integral part of the comparison field, in which case the comparison-value of the first stimulus becomes a function both of time and of the magnitude of the secondary stimulus relative to the first comparison-stimulus*" (25, p. 297; italicized in original).

Further experimentation led Pratt to suggest that general levels of reference, built up in the past, influence present stimuli (26). There would be a level of reference for any homogeneous group of impressions, such as impressions of auditory intensities. Every auditory stimulus that is presented to an observer is brought into relation with this level and as a result the judgment will very likely be influenced in some way. If a sound is above this level, it will receive a "still further enhancement in the louder direction" (26, p. 808); and similarly, when it is below, the judgment of its strength will suffer a further decrease. Thus the tone is judged in the opposite direction to the level of reference which influences it; this is usually referred to as contrast, while assimilation is the closer approximation of the stimuli that influence each other.

Needham has recently shown that if a series of tones precedes another series of a different intensity level, the former will influence the latter in the direction indicated by Pratt. In one situation Needham used three series at different intensity levels (23). The order of these series was so arranged that on some occasions each occurred first in an experimental session, and on other occasions second and third; that is, sometimes they were preceded by no other series, while on other occasions they were preceded by a weaker or stronger series. The judgments made when the series was first in the sitting were considered as the control or normal series; the judgments made on this same series when preceded by another series were compared with the normal series. By this technique Needham found that when a weak series is preceded by a loud series the variables of the former are judged weaker than they were under the normal condition. Likewise the variables of the loud series are judged louder if preceded by a weak series. If the intensity level of the series lies between the loud and the weak series, then its variables are judged louder when following the weak series and weaker when following the loud series, than they are judged when occurring first in an experimental sitting.

Fernberger has observed this same phenomenon in one of his many lifted weight experiments (9). He arranged the series so that certain critical weights (96 and 100 grams), which ordinarily provoke judgments that fall within the interval of uncertainty, followed (although not directly, since the standard always preceded the variable) the heaviest comparison weight (104 grams) of the series or the lightest comparison weight (88 grams). When the critical stimuli followed the heaviest weight a preponderance of lighter

judgments was found; but if they followed the lightest weight a preponderance of heavier judgments was found. Thus when either the 96 or 100 gm. followed the 104 gm. weight there was a "strong tendency" (9, p. 133) for it to be judged "lighter"; likewise if either of these two weights followed the 88 gm. weight there was "an equally strong tendency" (9, p. 133) for it to be judged "heavier." He concludes that there is "some sort of a contrast effect existing between the pairs of stimuli" (9, p. 145). He tentatively suggests, however, that there is set up a certain type of excitation which inclines the observer towards a reversal of his judgments from one pair to the succeeding pair; thus the implication is the establishment of a set producing reversals.

The phenomenon of contrast is, according to Hollingworth, the reverse of the phenomenon explained by the law of central tendency (16, p. 37). He found that the judgment of a variable of a series is not influenced so much by "its general relation to other members of the series or by the effect of an immediately preceding member as by its rather specific relation to the central tendency or mean or average of the series" (16, p. 37). Contrast would cause a weak variable, preceded by a strong one, to be judged still weaker while the law of central tendency requires that any such effect if present, be outweighed by the tendency for all judgments to assimilate towards the mean of the series, and consequently the weak variable should be judged stronger. This assimilation would cause weak variables to be over-estimated and strong variables to be under-estimated, with little or no error of estimation about the middle variables. In a set-up in which the series was from time to time extended, Hollingworth found that the "indifference point" (i.e., the point at which there is relatively little error of estimation) shifted as the boundaries of the series changed, always being near the mid-point of the series. He also found that at any one time the shortest variables were over-estimated; the longest, under-estimated. Thus a variable at the extreme upper end of the series at the beginning of the experiment was under-estimated, but as the series was extended this variable became the middle of the series, in which case the error of estimation was very small. With further extension of the series this variable became one of the shorter variables (relatively speaking) and the error became one of over-estimation.

The discussion above has been limited to the most prominent and pertinent theories that have been advanced to explain the occurrence of errors of estimation. They can be roughly classified into three

groups, but not without some overlapping. The theories of Fechner, Köhler, Lauenstein, and Woodrow can be placed together since some concept resembling assimilation is involved in all of them. Fechner's theory is simple and straightforward, without any physiological implications. Köhler's and Lauenstein's theories stress the physiological aspect, while Woodrow's emphasizes the psychological. Hollingworth's theory is not far removed from this group, but although the physiological implication is implicit, the theory is primarily psychological. It is not, however, as comprehensive a theory as the preceding. The second class involves the concept of "set," and into it fall the theories of Fernberger, and Martin and Müller. Finally a third type of theory, involving the principle of contrast, is offered by Pratt. In view of the diverse opinions concerning this perceptual phenomenon, it was thought that it would be of some value to obtain more experimental information, in the hope that some of the theories might be better evaluated. We were especially interested in resolving the opposition between the law of central tendency and the principle of contrast. To do this a detailed analysis of many judgments was planned. If judgments are made in absolute units, rather than in the conventional psychophysical categories, the average and standard deviation of the judgments can be easily obtained. These measures can be found for any, or all, of the data. Consequently by grouping the data in different ways the judgments can be studied under a variety of conditions. For instance, the average and standard deviation of the whole series can be computed as well as the average and standard deviation of the judgments for each variable; but an even more minute analysis is possible since these measures can be found for the judgments of each variable when preceded by every other variable. Because they promised a better understanding of the problem under investigation, such methods of analysis were used in the treatment of our data.

CHAPTER II

EXPERIMENTAL WORK

A. Apparatus

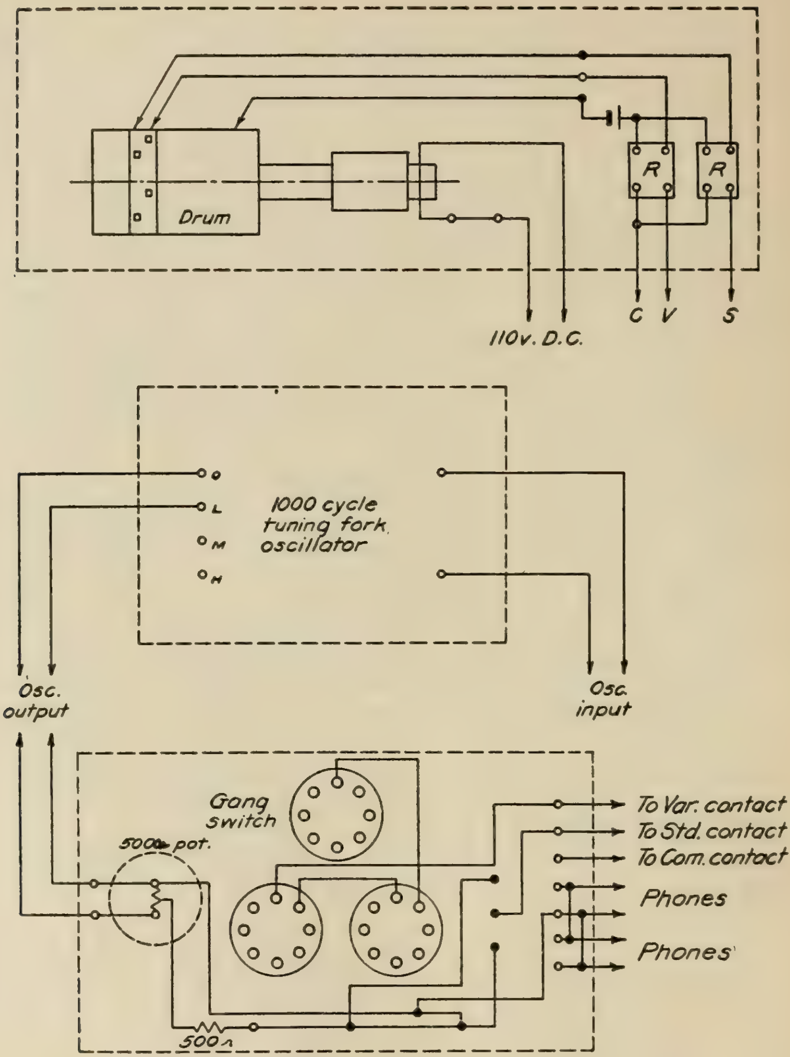
The apparatus used in this experiment consisted of an audio-oscillator, an attenuator, a timing device, and ear phones.

The audio-oscillator is a General Electric Co. product (type 213), with a constant frequency of 1000 cycles per second. It can be wired so that low, medium, or high degree of intensity can be produced; in this experiment the low intensity was used. This oscillator has been described in detail elsewhere (14). It was used by Kellogg and remains in the laboratory as part of the Kellogg set-up (17). It is inclosed in a sound-proof box.

The attenuator was built by an advanced engineering student. By using nicraloy wire of known resistance (i.e., so many ohms to the inch) it was possible to take off from the wire at various points and thereby obtain intervals of two decibels from 20–50 dbs., the range used in this experiment. The calibration of these steps was done very precisely by using a Wheatstone Bridge. All connections were soldered; then the wire was covered with sealing wax to prevent its being disturbed. These 15 points leading off from the nicraloy wire run to points on gang switches so that the various intensities can be produced by merely turning the dial to the point indicated. The attenuator also includes a 500 ohm resistance and a potentiometer; the latter makes it possible to adjust the attenuator for each observer so that the level used will be so many decibels above *his* limen.

The timing device consisted of a brass revolving drum against which contact points pressed. A stencil made of heavy paper[1] was perforated and shellacked in such a manner as to allow the two copper contact points to touch the drum at desired intervals. The stencil is of such a quality that it is not affected by the weather, nor do the contact points wear the paper down to any great extent. The drum is driven by a constant speed D. C. motor. As the current used for this motor is not absolutely constant it is to be expected that the speed varied slightly; but as a result of many recordings it is safe to state that the motor makes a complete revolution in $57\frac{1}{2}$ seconds $\pm \frac{1}{4}$ second.

[1] The writer would like to take this opportunity to thank Mr. Melvin Loos, of the Columbia University Press, for supplying this paper.



ATTENUATOR

Wiring diagram of the apparatus

The ear phones are a product of the Western Electric Co. (type 528) and are extremely sensitive. Only one of the phones used by the observer was connected with the apparatus, the other merely covering his ear. The experimenter also used an ear phone in order to check each sound as it came through.

The apparatus was in a relatively sound-proof room and when in

operation it made a constant faint vibration, which was slightly audible to the observer at the other end of the room. Preliminary work, however, indicated that when the phones are being worn this noise is practically eliminated.

It has been found that the loudness level in terms of just noticeable differences above the threshold is closely related to the logarithm of the intensity of the exciting stimulus. Consequently a logarithmic unit (decibel) has been adopted. The decibel is defined by the relation $L = 10 \log_{10} I_1/I_0$, where I_1 and I_0 are two different values of sound energy and L is the difference in energy level. More complete discussions of this unit will be found in the literature (11; 28).

The use of decibel units does not assume that the Weber-Fechner law applies to the sensation of hearing. To express results in decibels above a standard is merely to express energy in a logarithmic form. Nor does it involve characteristics of the ear (13), for it has been shown that at 1000 cycles per second a linear relation exists between loudness and sensation levels (1; 18).

There are about 37 distinguishable steps between 20 and 50 dbs. at a frequency of 1000 cycles (27). Since this is the range used in the present experiment and as the steps are two dbs., it is obvious that there are 2.3 J. N. D.'s between each two consecutive stimuli. Comparing this with other experiments, we find that Needham has on occasions used both one and two db. steps (23). Gage used steps of five dbs. in his work on limen determinations (12). As the number of psychological experiments using decibels is rather small, it would be well to compare our interval with those in other sensory fields. Oberlin reports that with a 300 gram weight the J. N. D. is 1/52 (6). If one uses this fraction at 100 grams, then there will be two (2.08) J. N. D.'s between 100 and 104. Thus in the usual psychological experiments of lifted weights two J. N. D.'s are used as step intervals. Some psychologists believe that even the four gram step is a little small; it has been suggested that steps of five grams would be more suitable. This would seem to indicate that the use of two db. steps is orthodox psychophysical procedure since the number of J. N. D.'s in two db. steps is approximately equal to that in the five gram step (2.3 J. N. D.'s as compared with 2.6 J. N. D.'s).

B. Procedure

1. Liminal Determinations

It will be obvious from the above discussion that the limen must be determined before the decibels above the threshold can be ob-

tained. The most efficient means available for the measuring of individual hearing capacity is the Western Electric Co.'s 2 A Audiometer, which uses decibel units of intensity levels. By means of this instrument each observer's limen was determined over a period of three days, so that the results may be presumed to give a fair indication of the individual limens. After the limen had been determined, the observer calibrated the attenuator against the audiometer. This was done by sounding the two instruments simultaneously, each ear receiving a sound from one of them. The audiometer was set to produce a tone of 1024 cycles per second and the intensity was a certain number of decibels above the threshold of the observer. For instance, the intensity of the audiometer set at 30 dbs. was compared with the intensity of the tone coming from the attenuator when set at 30 dbs. The loudness of the latter, which can be regulated by the potentiometer, was varied in the manner required by the method of limits. Thus we were able to find at what position the potentiometer should be placed in order for the observer to hear a sound equal in intensity to the one produced by the audiometer. To check this the same procedure was repeated with both the audiometer and the attenuator set at 40 dbs. above the limen. We therefore feel justified in saying that we are able to state with accuracy how many decibel units above the observer's threshold are the stimuli used in the experiment.

2. *Preliminary and Main Experiment*

The following typewritten instructions were given to the observer after the calibration of the attenuator had been completed:

PLEASE READ CAREFULLY AND ASK ANY QUESTIONS ON ANYTHING NOT FULLY UNDERSTOOD

This will be a long and tedious experiment, but a constant attitude is essential. To make conditions easier the observer is asked to report any change of attitude or any feeling of fatigue. The experimenter wishes the O to feel free to make comments on the procedure, apparatus, conditions of experiment, etc.

You will be given two tones, each of which will be sounded for a very short time. Between each two pairs of tones there will be a relatively long interval. It is desired that these pairs be kept distinct by the observer in making his judgments.

The tones that you will hear will vary in intensity *only*. The first sound will be called the standard, the value of which will change from time to time, but the subject will *always* be told *beforehand* when it will change. Thus the value of this tone will always be known. The second stimulus will vary around the standard; that is, sometimes it will be louder than the first, sometimes equal to the first, sometimes softer than the first.

After the second tone of each pair, the observer is to mark on a graph, which will be presented to him, the value of the second tone. For instance, if the first tone is known to be 30 and the second tone is judged to be 22, then the observer places a pencil mark on the line which is 22 on the graph.

It is desirable for the observer to always use the same ear and to always try to adjust the ear phones to the same tightness each time.

As indicated above the observer was instructed to mark his response on graphs which were mimeographed on sheets of paper, containing 25 lines to the sheet. The lines were divided in the following manner:

It will be seen that the judgments were in absolute units. Since it has been shown (7; 24) that threshold determinations, as well as measures of variability obtained by this method, are comparable to those obtained by the usual method of constant stimuli, we could use *either* if we were interested in limens; but, as a detailed analysis of the data was planned, it seemed preferable to have each judgment estimated in absolute units rather than in the three conventional categories. This permits of a treatment of the data by ordinary statistical methods, as well as by the psychophysical methods. Consequently the mean judgment for each variable, as well as a measure of dispersion, can be calculated. In addition, this method makes it possible for the judgments of each variable to be studied under various conditions; for example, the influence of preceding variables can be determined by finding the average of the judgments of a variable when preceded by one, and then another, variable. Thus it was believed that judgments in absolute units would allow a more thorough analysis of the problem under investigation.

Two standards were used: 30 and 40 decibels above the observer's limen. The series in which these standards were used followed each other an equal number of times. For one half of the observers the series using 30 as the standard was presented first in an experimental sitting, the 40 series following after an interval of about five minutes. The other half of the observers started with the 40 series which in turn was followed by the 30 series. In the next experimental sitting the order was reversed.

The range of variables used extended ± 10 dbs. for each standard; that is, the 30 series extended from 20 to 40 dbs., in steps of two

dbs.; the 40 series from 30 to 50, in steps of two dbs. The order of the variables was arranged so that in each series every variable followed and preceded every other variable (9).

The stimuli were sounded for one-half second, and the stimuli of a pair were separated by one-half second. The pairs of stimuli were separated by an interval of ten seconds. Thus:

standard	interval	variable	interval	standard
½ sec.	½ sec.	½ sec.	10 sec.	½ sec.

This relatively long interval between the pairs of stimuli was selected for several reasons: in the first place, it would be poor experimental technique to create conditions which would load the dice in favor of contrast effect when the problem is to determine whether contrast effect, central tendency, or either operates. By lengthening the interval the possibility of the separation of O's judgments into pairs is increased, and consequently the tendency to carry over from one stimulus of a pair to a stimulus of the succeeding pair is reduced. In the second place, judging in absolute units is a tedious job and the observer should be allowed a little more time than he is ordinarily given. Finally, the interval also tends to reduce fatigue.

Using the arrangement of presentation indicated above, 11.5 seconds were required for a pair of stimuli (from first standard to second standard) to be given. This allowed five judgments a minute. With 11 categories in each series and with each variable preceding and following every other variable of the series, we have a total of 110 arrangements in each series. Thus, each sitting consisted of 220 judgments—110 for each of the two standards; the actual presentation time was 44 minutes, but a pause at the end of 22 minutes made the sitting approximately an hour long.

Only one time order was used. It has been proven that the time error is not due to the use of paired stimuli (26) and for the purpose of this experiment it was more important to collect judgments with only one time order.

Six observers, all naïve, were used in the experiment: two in the preliminary work and four in the main experiment. The nature of this investigation and the set-up used made it advisable that a small number of observers be used, and that many judgments be obtained from each. Consequently each observer had 18 experimental sittings[2] with 220 judgments in each, making a total of 990 judgments

[2] There is one exception to this—one observer in the preliminary part had only nine sittings.

from each observer under each of the four conditions: the 30 series presented first in an experimental sitting, the 30 series presented second (i.e., following the 40 series), the 40 series first, and the 40 series second.

It was possible to plan the above procedure, but preliminary experimentation was necessary to determine several points:

1. The advisability of using the method of "absolute units" when dealing with discrimination in terms of decibels;
2. Use of step intervals of two decibels;
3. The appropriateness of the system of recording judgments;
4. The question of orientation.

At this point graduate students in psychology, experienced in psychophysical work of the kind involved, rendered valuable assistance. Their record sheets indicated that they were able to discriminate between the various intensities with a great deal of accuracy, but in their introspections they stated that close attention was necessary if such discriminations were to be made. Since the experiment was not concerned with limens of any type, the sole requirement was that the variables be separated by such intervals as to make discrimination possible only by close attention on the part of the observer. It was believed that the two decibel steps were appropriate.

The sophisticated observers also found the system of recording judgments satisfactory as well as agreeable.

On the other hand, the question of the observer's orientation within the stimulus range could be answered only by many sittings such as would be used in the main experiment. Since naïve observers were to be used in the latter, it was thought desirable to use them in the preliminary work.

After the instructions were read, a short orientation period was given in which the experimenter sounded the standard and the variable tones. Subsequent to this the observer was asked to make his judgments verbally on each pair of stimuli. When it was evident that the scale was fixed in the observer's mind the series began.

During the first few sittings the observers inquired as to the limits of the series. In the preliminary work the experimenter indicated that the 30 series extended from 20–40 and the 40 series from 30–50. Thus the limits of the series were known to these observers; but this procedure was found unsatisfactory (see below), and consequently another was adopted for the main experiment.

As the observers were required to make absolute judgments it seemed necessary to give more orientation than is usually done in

this type of work. It was therefore decided that each O would be given a short orientation period on the first day, similar to that given in the preliminary work, plus a very general orientation on each succeeding day. This consisted of six different tones being sounded: 10, 20, 30, 40, 50, and 60 dbs. The observer knew that these particular tones would be sounded along with a known standard, but he did not know in what order they were given. He responded verbally and each of the six tones was repeated until he had correctly identified them all (naturally the Os were ignorant of this procedure). It was seldom necessary to repeat more than one or two tones for any one sitting. Thus the total range from which the variables were selected was well established, and consequently when an observer asked about the limits of the series he was told that he should be able to decide that for himself since all the variables were selected from within the range of tones sounded for him at the beginning of each sitting; but at no time was he led to believe that any series extended as far down, or as far up, as the tones sounded at the opening of each experimental session.

CHAPTER III

RESULTS

A. Preliminary Experiment

In the results throughout this experiment the series which was presented first in an experimental sitting is taken as the norm or control series. The series that was presented second in a sitting, that is, after a series of more intense or of less intense stimuli, is considered the experimental series, since it may be affected by the preceding series of a different intensity level. Using this method, Table 1 shows that the means for both the 30 and the 40 series are

TABLE 1

The Means of the Judgments of Two Os in the Preliminary Experiment Under Four Conditions

Conditions	*Means*
30 series presented first in the sitting	29.94
30 " " second " " "	30.51
40 " " first " " "	40.09
40 " " second " " "	40.54

higher when they are presented second than when presented first. Such results in the 30 series conflict with the principle of contrast effect, for according to it the mean for this series, when following a louder series, should be lower than the mean for the control series. But the results of the 40 series can be said to show evidence of contrast, since a weak series preceding the 40 series pushes the mid-point of the latter up a little higher than that of the control series. Thus the tones in the 40 series are judged higher when they are presented after the 30 series.

These data indicate that the preceding series has some effect on the series that follows, but the nature of this effect is not consistent. The mere fact, however, that it is present makes it justifiable to present data that pertain to the two series when they occur first in the sitting. Thus Table 2 deals only with the control series, showing the time error for each of the 11 variables in both series. It will be seen that all judgments tend to approach the average of the series; the louder variables are judged weaker than they objectively are and the weaker variables are judged louder, with little or no error around the mid-point of the series. It will be recalled that the limits of the

TABLE 2

Time Error for Each Variable in the 30 and the 40 Series when Presented First in an Experimental Sitting. (Av. of Two Os.)

30 Series First		*40 Series First*	
Vs.	*T. E. for Each V.*	*Vs.*	*T. E. for Each V.*
20	−1.37	30	−1.88
22	− .56	32	−1.19
24	− .29	34	− .76
26	− .13	36	− .46
28	− .01	38	− .13
30	+ .09	40	− .28
32	+ .36	42	+ .47
34	+ .37	44	+ .50
36	+ .66	46	+ .61
38	+ .44	48	+ .89
40	+1.03	50	+1.21

series were known to these observers and it is believed that this fact can be used to account for the results obtained.

If the objective limits are known to the observer, it is very likely that no tone will be estimated above or below these points. Consequently, it is almost impossible for the series to expand; rather it contracts at both ends. All of the variables must be made to fit into the boundaries of the series, and if there is any error in the estimation of the variables it would be such as to make the smaller variables larger, and the larger ones, smaller.

Further evidence for the above explanation can be found in the number of judgments above or below the objective limits. It should be noted that the results quoted are those of one observer, who stated in his introspections that occasionally he believed the tones to be above or below the limits of the series, but hesitated to mark them that way, since the series extended only so far. This remark was made after the 13th sitting. At the beginning of the next sitting, and throughout the remainder of the experiment, the six tones (10, 20, 30, 40, 50, and 60) used in the day to day orientation in the main experiment were sounded and the observer was told that the limits might not be the same as they had been. At the end of the experiment he admitted that for the first day or so after the new procedure he believed the limits had changed, but he soon felt that the experimenter was only "playing a joke on him," and reestablished the mental set for the old boundaries. An inspection of Table 3 shows a large increase in the number of judgments given below or above the objective limits for the 14th sitting and adds proof to the state-

TABLE 3

NUMBER OF JUDGMENTS ABOVE OR BELOW THE OBJECTIVE LIMITS OF THE SERIES. (FOR ONE O; UNDER ALL FOUR CONDITIONS.)

Sitting	*Number of Judgments*	*Sitting*	*Number of Judgments*
1st	46	10th	0
2nd	8	11th	0
3rd	5	12th	0
4th	1	13th	2
5th	0	14th	31*
6th	0	15th	11
7th	0	16th	13
8th	0	17th	8
9th	0	18th	0

* Instructions changed at beginning of the 14th sitting.

ment that if the limits are known, the number of judgments beyond these points is greatly reduced.

A further check on this explanation was made by using the method of orientation employed in the main experiment and switching to that of the preliminary experiment when the main one was completed. Table 4 gives the results obtained from one observer by

TABLE 4

SERIES IN WHICH LIMITS ARE UNKNOWN COMPARED WITH SERIES IN WHICH LIMITS ARE KNOWN. (ONE OBSERVER; DATA FROM 40 SERIES PRESENTED FIRST.)

Series with Limits Unknown		*Series with Limits Known*	
Vs.	*Time Error for Each Variable*	*Vs.*	*Time Error for Each Variable*
30	+ .47	30	−2.90
32	+1.04	32	−1.92
34	+ .86	34	−1.44
36	+ .89	36	− .60
38	− .04	38	− .12
40	+ .19	40	+ .57
42	+ .48	42	+1.14
44	+ .12	44	+1.28
46	− .41	46	+1.04
48	−1.80	48	+ .96
50	−1.92	50	+1.38
Av. for all Vs.	− .01	Av. for all Vs.	− .06

both procedures for the 40 series when it is presented first. When the limits are known we find an over-estimation of the weak variables and an under-estimation of the loud variables, but when the limits

are unknown the reverse is found (i.e., under-estimation of the weak and over-estimation of the loud variables).

From these results it is evident that working with knowledge of the limits of the series predetermined, at least to some extent, the form that the data should take. Consequently, this knowledge was withheld in the main experiment, substituting for it the day to day orientation described above.

B. Main Experiment

The purpose of this experiment is to investigate the effect of the preceding stimulation upon the succeeding stimulation. Consequently, we must examine the data obtained under all of the following conditions, since an effect might appear in any one, or all of them. (1) *A series preceded by another series of different intensity level.* This analysis consists of comparing the data for the 30 series when presented first in an experimental sitting with those for the 30 series when presented after the 40 series. The same technique is applied to the 40 series. In each case the comparison is between the series when presented first in a sitting and the same series when presented second in a sitting. (2) *A variable preceded* (*although not directly*) *by another variable.* This consists of an intra-serial analysis of the judgments for each variable of the series when preceded by every other variable of that series. (3) *A variable preceded by different standards.* This involves an analysis of the judgments for the variables 30 to 40 that appear in both series. Here we are afforded an opportunity to study the effect of different standards upon the same variable.

(1) A series preceded by another series of a different intensity level

The effect of the 40 series on the 30 series can be studied by comparing the mean of the 30 series when it is first in the sitting with its mean when it is second in the sitting. Table 5 presents these means along with those of the 40 series under similar conditions. Presenting the more intense (40) series before the less intense (30) caused the mid-point of the latter to rise slightly. Thus the variables of the 30 series are judged louder as a result of the 40 series preceding it. But the variables of the 40 series are also judged louder as a result of being presented after a *weak* series. The difference between the two averages of the 40 series has been found to be statistically reliable, but this is not true of the 30 series. Thus in

TABLE 5

AVERAGE, STANDARD DEVIATION, AND CRITICAL RATIO OF THE JUDGMENTS OF THE VARIABLES OF THE 30 AND 40 SERIES WHEN PRESENTED FIRST AND SECOND IN AN EXPERIMENTAL SITTING. (AV. OF FOUR OS.)

Position in Experimental Sitting	*Average*	*Standard Deviation*	*Critical Ratio*
30 series first	30.16	8.25	1.11
30 series second	30.36	7.77	
40 series first	39.26	8.62	3.21
40 series second	39.87	8.02	

(N = 3960 for each series under each condition)

one of two cases a preceding series does have enough effect on the following series to cause the averages of the series to be reliably different; in this case the average is raised by the preceding series. This change could very well be explained by the principle of contrast if one of the two following results had been obtained: (1) a lowering of the average of the 30 series when preceded by a more intense series; (2) a statistically reliable difference between the two means in the direction just indicated. The shift was found to be in the opposite direction. Consequently no definite conclusions can be drawn in regard to the effect of a series of different intensity on a succeeding series. The results from the preliminary experiment, since they agree essentially with the above, add support to such a position.

Since the effect on the average of all eleven categories of a series is small it might be well to see if this effect is approximately the same for all variables, or whether it is greater for some than for others. Tables 6 and 7 allow such an analysis to be made. When the two 30 series are compared we find a shift in the direction required by the principle of contrast for three of the more intense variables, but in the opposite direction for the remaining variables; but in the 40 series the mid-point of every variable, except 44, is thrown up as a result of the weak series preceding it. This is indicative of contrast. It will be noticed that six of the critical ratios are reliable and another practically so. Five of these seven show the contrast effect. It will be observed that the effect of the preceding series is most pronounced at the extremes of each series and considerably less around the mid-point of each series. At three of the four extremes the shifts in the means are indicative of contrast effect. At the lower extreme of the 30 series the shifts are in the opposite direction from what contrast demands and in two cases

TABLE 6

AVERAGE, STANDARD DEVIATION, AND CRITICAL RATIO FOR EACH OF THE VARIABLES OF THE 30 SERIES WHEN PRESENTED FIRST AND SECOND IN AN EXPERIMENTAL SITTING. (AV. OF FOUR OS.)

Variables	*30 Series Presented First*		*Critical Ratio*	*30 Series Presented Second*	
	Average	*Standard Deviation*		*Average*	*Standard Deviation*
20	19.65	3.88	4.48	20.95	3.85
22	21.17	3.68	2.00	21.71	3.51
24	23.06	3.51	3.52	24.01	3.84
26	25.17	3.58	1.77	25.63	3.31
28	27.76	3.05	1.59	28.11	2.82
30	29.68	2.63	.58	29.79	2.44
32	31.24	3.15	1.00	31.01	2.99*
34	33.66	3.66	.07	33.68	3.66
36	37.01	4.25	0	37.01	4.07
38	40.28	4.12	.94	39.99	4.16*
40	43.08	4.19	2.94	42.11	4.65*

(N = 360 for each variable)
* These cases agree with the principle of contrast.

these changes are reliable. It should be noticed that the variables in this region of the 30 series second are judged more accurately than the corresponding ones of the 30 series first.

Thus in thirteen instances out of twenty-two we find shifts in accordance with contrast. If percentages were used in determining

TABLE 7

AVERAGE, STANDARD DEVIATION, AND CRITICAL RATIO FOR EACH OF THE VARIABLES OF THE 40 SERIES WHEN PRESENTED FIRST AND SECOND IN AN EXPERIMENTAL SITTING. (AV. OF FOUR OS.)

Variables	*40 Series Presented First*		*Critical Ratio*	*40 Series Presented Second*	
	Average	*Standard Deviation*		*Average*	*Standard Deviation*
30	27.48	4.72	5.94	29.38	3.68*
32	29.47	4.12	4.72	30.84	3.70*
34	31.84	4.01	3.29	32.76	3.44*
36	33.92	3.98	3.71	34.96	3.60*
38	37.07	3.29	2.04	37.56	3.12*
40	39.46	2.54	.63	39.58	2.66*
42	40.99	2.79	.85	41.16	2.64*
44	43.77	3.65	.27	43.70	3.36
46	46.48	3.89	.93	46.73	3.32*
48	49.44	3.66	.38	49.54	3.34*
50	51.93	4.19	1.52	52.37	3.62*

(N = 360 for each variable)
* These cases agree with the principle of contrast.

whether contrast operates, we could say that it is more likely to appear than an opposite effect. Of the seven critical ratios which are reliable (all over three except one, which is 2.94) five are among the thirteen cases mentioned above. Using this as a criterion it could be stated that the presence of a contrast effect is strongly indicated.

The mere fact that the preceding series does have some effect makes it essential that only the results from the control series (i.e., the series presented first in an experimental sitting) be considered in the remainder of our investigation, since the shifts in the averages can not be ruled out of the data obtained from the series presented second in a sitting. However, occasional reference will be made to the results of the latter series since it appears that they substantiate some of the findings reached by an analysis of the data of the control series.

(2) A variable preceded by another variable

As indicated above it is also possible that variables might influence one another; such an effect can be isolated to some extent by making an analysis of all the judgments for every variable after every other variable. Such an intra-serial analysis will be found in Tables 8 and 9. The average judgment for each variable when preceded by every other variable of that series is presented in the first part of each of these tables. For example, when the variable 20 was preceded by 22, the average judgment for 20 was 18.47; when 20 was preceded by 24, the average judgment for 20 was 19.56. (Graphical presentation of these data will be found in Figs. 1 and 2, but the time error has been plotted rather than the average merely as a matter of convenience.)

In the second part of each table we find the standard deviation of the judgments for each variable when preceded by every other variable; they are read in the same manner as the averages. (They are graphically presented in Figs. 3 and 4.) These standard deviations have been obtained by throwing the data from the four observers together and treating them as one distribution, rather than as four. To test the validity of this procedure a method of linear interpolation was employed to find the standard deviation of each observer under each condition; these standard deviations were averaged and are presented in Figs. 3 and 4. It will be seen that the standard deviations calculated by the two methods differ only in absolute magnitude. On the whole the use of individual standard

TABLE 8

AN ANALYSIS OF ALL JUDGMENTS FOR EACH VARIABLE WHEN PRECEDED BY EVERY OTHER VARIABLE OF THE 30 SERIES PRESENTED FIRST IN AN EXPERIMENTAL SITTING. (AVERAGE OF FOUR OS.)

Average of Judgments for Variables at Left when Preceded by Every Other Variable

Variable Judged	*Preceding Variable*										
	20	*22*	*24*	*26*	*28*	*30*	*32*	*34*	*36*	*38*	*40*
20		18.47	19.56	19.50	19.11	21.36	19.06	19.50	19.78	20.31	19.83
22	20.64		19.89	20.56	20.33	20.89	21.86	21.58	23.03	21.94	20.94
24	22.83	22.89		22.44	22.86	23.00	23.83	23.28	23.25	22.78	23.47
26	24.28	25.50	24.14		25.08	25.69	25.56	25.03	25.50	25.53	25.42
28	26.36	27.50	27.58	27.75		28.25	28.42	28.17	27.17	28.50	27.89
30	29.31	29.42	29.92	29.08	29.61		29.42	30.17	29.86	30.03	30.00
32	30.67	31.83	31.64	30.89	32.33	30.44		31.25	31.44	31.22	30.69
34	33.92	33.39	33.42	33.19	33.81	33.28	33.64		34.22	34.44	33.31
36	37.50	37.39	37.36	37.72	36.81	36.67	36.42	37.28		37.61	35.31
38	40.83	40.69	40.67	40.72	39.25	39.03	40.08	40.58	40.81		40.17
40	43.78	43.53	42.50	42.97	39.89	43.22	43.25	43.22	44.56	43.89	

Standard Deviation of the Judgments for Each Variable when Preceded by Every Other Variable

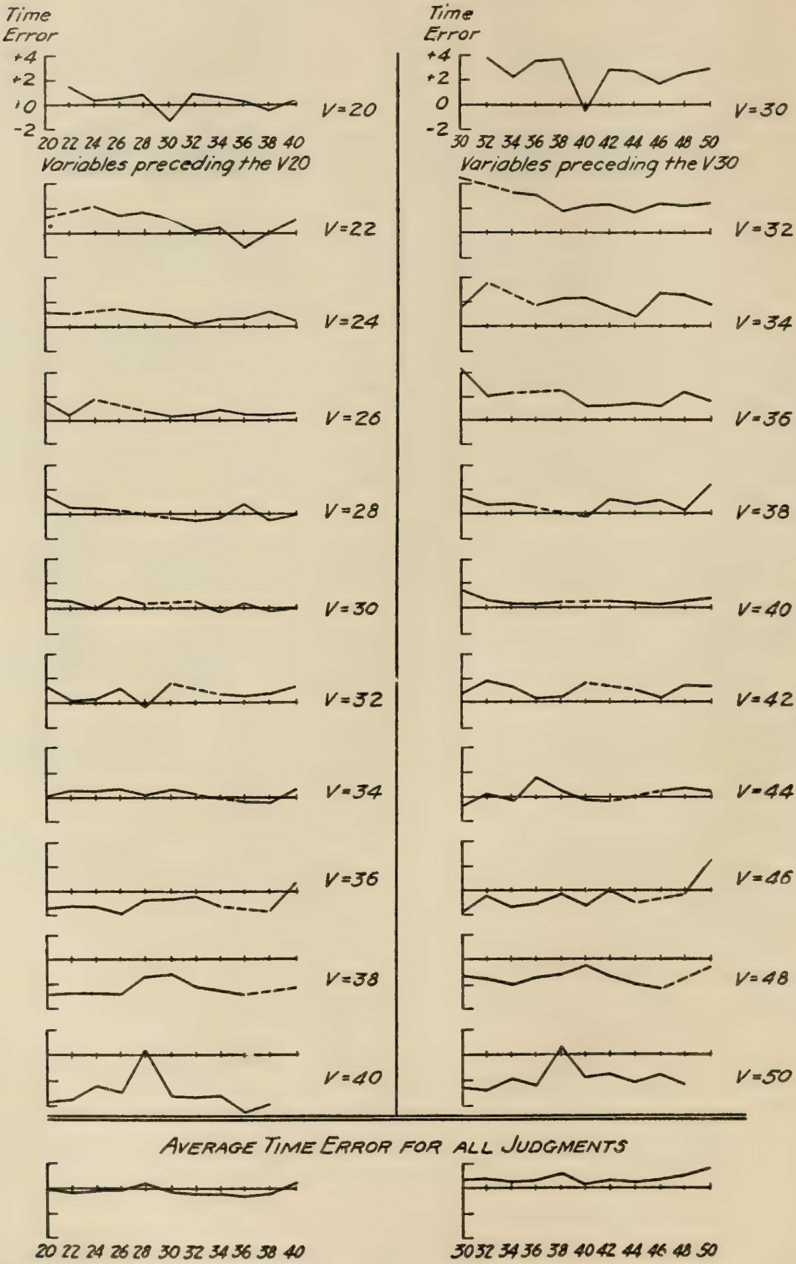
Variable Judged	*Preceding Variable*											
	20	*22*	*24*	*26*	*28*	*30*	*32*	*34*	*36*	*38*	*40*	*Average*
20		3.81	4.41	3.80	4.39	3.94	3.37	2.96	3.02	4.50	3.70	3.78
22	3.84		4.24	3.01	3.10	2.93	3.23	3.12	3.59	4.67	3.82	3.56
24	3.39	4.06		4.62	2.81	3.37	3.24	2.89	3.25	3.08	3.57	3.43
26	3.56	3.34	3.78		3.03	2.87	4.26	2.97	3.61	3.96	3.61	3.50
28	4.39	2.57	2.89	2.63		2.42	2.58	2.78	2.75	2.64	3.73	2.94
30	2.25	1.94	3.32	2.62	2.15		2.36	3.07	2.95	2.38	2.53	2.56
32	3.75	3.06	3.55	2.54	3.45	3.11		2.66	2.69	2.87	3.09	3.08
34	3.05	4.14	3.43	2.99	3.17	2.90	3.21		3.57	5.16	4.00	3.56
36	4.27	3.66	3.31	3.47	4.01	3.09	3.48	4.67		6.40	4.52	4.09
38	4.30	4.49	3.97	3.73	3.74	3.36	3.58	3.96	4.82		4.24	4.02
40	3.57	4.06	4.16	3.74	3.31	3.82	4.39	4.36	4.04	4.50		4.00
Average	3.64	3.51	3.70	3.32	3.32	3.18	3.37	3.34	3.43	4.02	3.68	3.50

TABLE 9

An Analysis of All Judgments for Each Variable when Preceded by Every Other Variable of the 40 Series Presented First in an Experimental Sitting. (Average of Four Os.)

Variable judged	*Average of Judgments for Variables at Left when Preceded by Every Other Variable*										
	Preceding Variable										
	30	*32*	*34*	*36*	*38*	*40*	*42*	*44*	*46*	*48*	*50*
30		26.22	27.81	26.56	26.36	30.56	27.19	27.25	28.17	27.58	27.08
32	27.72		28.67	28.92	30.17	29.94	29.61	30.28	29.69	29.86	29.83
34	32.31	30.42		32.14	31.75	31.64	32.31	33.11	31.22	31.39	32.17
36	31.83	33.97	33.61		33.33	34.78	34.72	34.47	34.72	33.58	34.14
38	36.50	37.31	37.08	37.53		38.08	36.78	37.03	36.97	37.72	35.67
40	38.53	39.36	39.78	39.81	39.64		39.42	39.53	39.78	39.50	39.25
42	41.11	40.17	40.72	41.78	41.64	40.42		41.00	41.67	40.64	40.75
44	44.94	43.92	44.11	42.33	43.56	44.14	44.28		43.69	43.22	43.53
46	47.81	46.53	47.36	47.03	46.19	47.17	46.00	47.03		46.36	43.33
48	49.33	49.67	49.97	49.42	49.17	48.47	49.36	49.97	50.39		48.67
50	52.67	52.92	51.97	52.42	49.58	51.94	51.61	52.17	51.69	52.33	

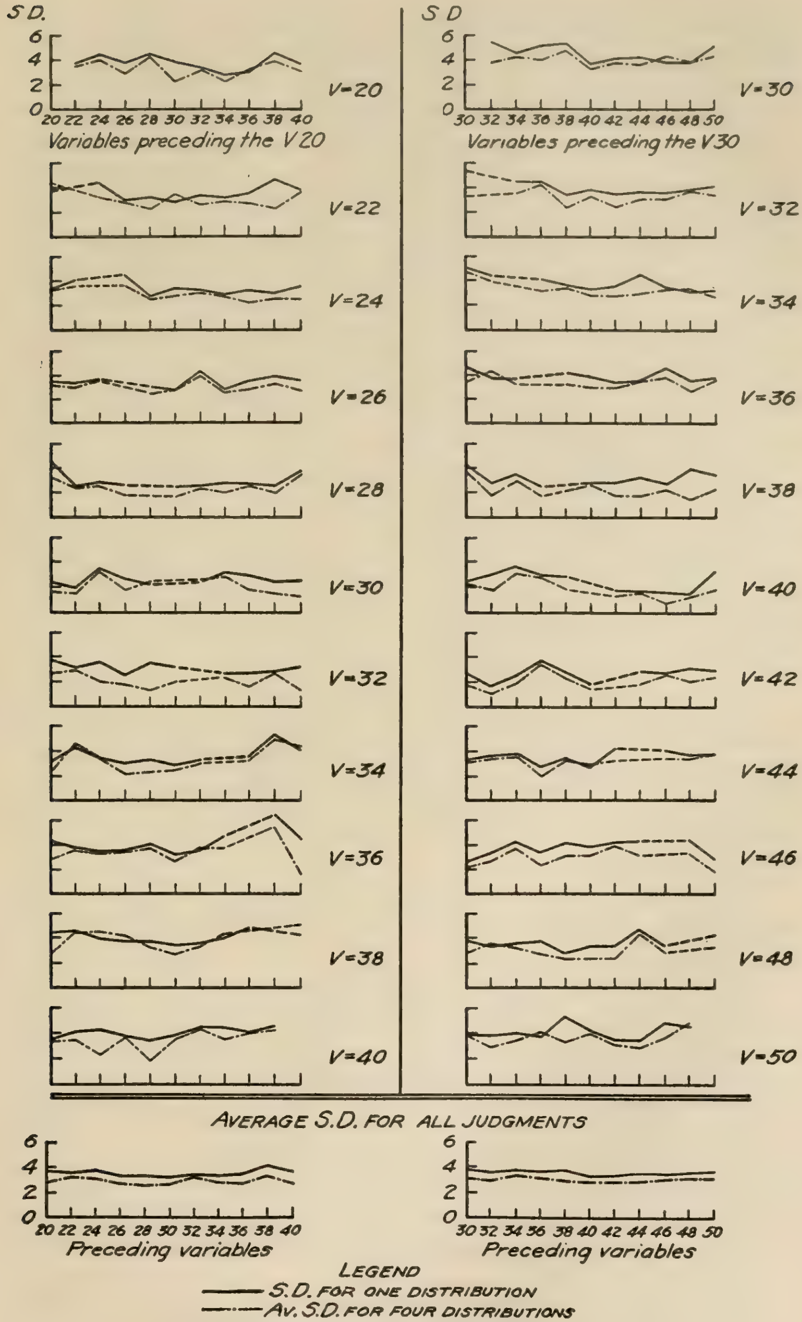
Variable judged	*Standard Deviation of the Judgments for Each Variable when Preceded by Every Other Variable*											
	Preceding Variable											
	30	*32*	*34*	*36*	*38*	*40*	*42*	*44*	*46*	*48*	*50*	*Average*
30		5.54	4.64	5.14	5.28	3.77	4.06	4.13	3.87	3.96	5.01	4.54
32	5.35		4.56	4.47	3.41	3.95	3.44	3.70	3.59	3.69	4.01	4.02
34	5.04	4.47		4.14	3.69	3.35	3.61	4.55	3.39	3.11	3.19	3.85
36	4.77	3.82	3.65		4.12	3.90	3.40	3.51	4.55	3.51	3.79	3.90
38	4.11	2.89	3.41	2.51		2.93	2.93	3.24	2.73	3.99	3.50	3.22
40	2.39	2.98	3.59	2.84	2.88		1.73	1.44	1.56	1.40	3.14	2.40
42	2.77	1.77	2.46	3.60	2.73	1.88		2.97	2.65	3.05	2.99	2.69
44	3.22	3.58	3.80	2.83	3.21	2.89	4.18		4.08	3.73	3.87	3.54
46	2.63	3.24	4.10	3.39	4.08	3.71	4.03	4.17		4.33	2.86	3.65
48	3.79	3.33	3.46	3.64	2.84	3.20	3.20	4.52	3.34		4.21	3.55
50	3.89	3.86	4.00	3.66	5.14	4.05	3.35	3.39	4.76	4.48		4.06
Average	3.80	3.55	3.77	3.62	3.74	3.36	3.39	3.56	3.45	3.53	3.66	3.58



The time error of the judgments

FIG. 1. Data from Table 8
30 series

FIG. 2. Data from Table 9
40 series



Standard deviation of the judgments

FIG. 3. Data from Table 8
30 series

FIG. 4. Data from Table 9
40 series

deviations does not increase the smoothness of the graphs to any great extent; they present the same general picture as the standard deviations calculated from the combined data of the four observers.

In inspecting the curves of page 32 we can examine whether those for the 30 and 40 series are similar in slope and other characteristics in corresponding situations, i.e., as one reads horizontally across the chart from Fig. 1 to Fig. 2. We might expect opposite pictures at the two extremes of the same series, and the same pictures at corresponding parts of the two series. We actually find this expectation borne out in the extremes. All four curves show a pronounced eccentricity at or close to the middle of the series. The time error for V 20 in the 30 series, and for V 30 in the 40 series, takes a sudden tumble when the preceding variable was 30 and 40 respectively. At the other extreme, the time error takes a sudden jump in the positive direction, not exactly at the point where the preceding variable was 30 and 40 respectively but close to those points. A possible explanation is that the shift of time error in these instances amounts on the whole to a closer approach to accurate judgment; judgment of the extreme variables is most correct when the preceding variable is equal to the standard, or very close to it.

The curves for V 30 in the 30 series and for V 40 in the 40 series are alike in being the flattest and in showing the smallest time error. We conclude that a variable which is identical with the standard is most accurately judged and is least dependent on the preceding variable; it shows the smallest variation with changes in the preceding variable.

The remainder of the curves are not so definite; yet it seems fair to accept a downward slope to the right of the upper few curves in each series.

In the above paragraphs we are concerned with the effect of the preceding variable upon each individual variable. But we can also find, by combining the average obtained for each variable when preceded by any one variable, an average effect of any preceding variable on all immediately succeeding ones; thus the averages in Tables 8 and 9 are considered horizontally. (These are presented in the last graph of Figs. 1 and 2.) Such an analysis points out that the error of estimation tends to be less when the preceding variable is near the mid-point of a series, with the exception of the weaker variables of the 30 series.

The standard deviations in Tables 8 and 9 have been averaged

horizontally and vertically. When averaged horizontally we have an *average* standard deviation for each variable of each series. These can be compared with the standard deviations of the corresponding variables obtained by grouping all the judgments for any one variable together (i.e., disregarding preceding variables) and treating as one distribution (see Tables 6 and 7). The absolute value of the standard deviations is reduced by averaging them, but the relative position is identical (except for one case, the variable 36 of the 40 series first). We find that the standard deviations are larger at the extremes of the series than at the mid-point, with a gradual decrease from extremes to mid-point. In the 30 series there are two inversions (variables 26 and 36); these occur in the S. D. of the single distribution as well as in the average S. D. In the 40 series there is only one inversion (variable 46) when the standard deviations calculated from the single distribution are considered, but two inversions (variables 36 and 46) when the average standard deviations are considered.

Following down the separate columns of S. D. values in Tables 8 and 9 we find the same tendency as in the averages at the right. Without regard to the preceding variable it can be said that the S. D. tends to be smallest in judging the variables at and near the middle of the series. These middle variables are most stably estimated. Additional evidence of this can be found by considering the standard deviations of the judgments for the variables of the 30 and 40 series when presented second in a sitting (see Tables 6 and 7). Here, too, we find that the variables near the middle of the series have the smallest scatter of judgments.

By averaging the standard deviations in Tables 8 and 9 vertically we find a similar result; i.e., the averages are larger at the extremes of each series and decrease in magnitude as the mid-point is approached, the middle variable of each series having the smallest standard deviation. The standard deviation is smallest when the preceding variable has been at, or near, the average of the series.

Thus we find that when the preceding tone is the same as the standard, the variable is judged, on the average, with less error of estimation. Furthermore, the variability of such judgments is less. For example, if the tones are 30 (standard) : 30 (variable) : 30 (standard) : 20 (variable), the variable 30 has a stabilizing effect on the estimation of the succeeding variable 20; under such conditions the accuracy and consistency of the judgment will be greater than if the preceding variable had been, for example, 22 or 40. In addi-

tion, we find that when the succeeding variable is near the mid-point of the series both error of estimation and variability are small, regardless of what the preceding variable may have been. This justifies the statement that whenever a variable near the mean of the series is presented, not only will it be consistently judged accurately, but moreover, it will increase the accuracy and consistency of the judgment of the next variable.

(3) A variable preceded by different standards

The last condition where one stimulus might influence another is in the relationship between the standard and the variable. The standard differed in the two series, being 30 and 40 dbs. respectively. Certain of the variables, 30 to 40, were the same in the two series. The results for these variables should show the effect of the difference of standard. In the 30 series the variables 30, 32, and 34 occur in the middle of the series. They are, as would be expected, estimated very accurately, but with a tendency to be slightly under-estimated (see Table 10; Fig. 5). However, when these same vari-

TABLE 10
AVERAGE AND TIME ERROR FOR THE JUDGMENTS OF THE VARIABLES 30 TO 40 OF THE 30 AND 40 SERIES WHEN PRESENTED FIRST IN A SITTING.
(AV. OF FOUR OS.)

Variables	*30 series*		*40 series*	
	Average	*Time error*	*Average*	*Time error*
30	29.68	+ .32	27.48	+ 2.52
32	31.24	+ .76	29.47	+ 2.53
34	33.66	+ .34	31.84	+ 2.16
36	37.01	− 1.01	33.92	+ 2.08
38	40.28	− 2.28	37.07	+ .93
40	43.08	− 3.08	39.46	+ .54

(N = 360 for each V of each series)

ables occur in the 40 series as weak variables the error of under-estimation is considerably increased. Conversely, the variables 38 and 40 are the most intense variables of the 30 series and are greatly over-estimated; but when they occur in the 40 series in which they are the middle variables, they are under-estimated by a small amount. The variable 36 is over-estimated in the 30 series and under-estimated in the 40 series.

Thus we find that when these variables are presented after the standard 40 they are all under-estimated, but when they occur after

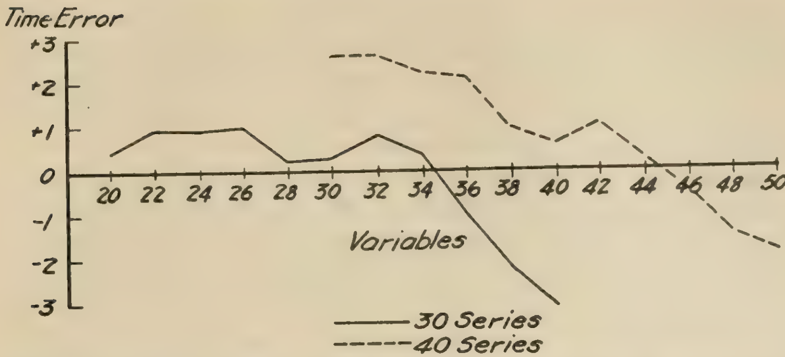


FIG. 5. Time error for each variable of the 30 and 40 series when these series were presented first in an experimental sitting. (Av. of four Os.)

the standard 30 this under-estimation is greatly reduced and in some cases the variables are even over-estimated.

From the above it follows that the variables at the lower extreme are under-estimated and those at the upper extreme over-estimated; little or no error occurs for those variables near the average of the series. Again attention might be called to the fact that the results from the series presented second in a sitting agree with those presented in Table 10.

Another measure of this effect may be obtained by the use of the following formula: $D = \frac{W - L}{N}$, where W is the number of "weaker" judgments; L, the number of "louder" judgments; N, the sum of W and L; and D, the preponderance of "weak" judgments when D is positive, or of "loud" when D is negative. This formula has been applied to the judgments for each variable between 30 and 40 of the 30 and 40 series when presented first in a sitting (Table 11).

TABLE 11

THE VALUE OF D PERCENT FOR THE VARIABLES 30 TO 40 OF THE 30 AND 40 SERIES WHEN PRESENTED FIRST IN A SITTING. (AV. OF FOUR OS.)

Variables	*D percent*	
	30 series	*40 series*
30	+31	+54
32	+49	+60
34	+9	+46
36	−25	+52
38	−53	+14
40	−67	+43

The data so obtained substantiate the results presented in Table 10 in that there is a preponderance of loud judgments for the variables 36, 38, and 40 when they occur in the 30 series. Similarly a shift from more "loud" to more "weak" judgments is also found when these variables occur in the 40 series.

Thus the data presented in Tables 10 and 11 supplement each other, although obtained by entirely different methods. But the two tables do not agree perfectly and since Table 10 gives an absolute measure of the direction and magnitude of the time error, the writer is inclined to prefer this method for studying contrast effect and related phenomena to that used to obtain the results of Table 11. The latter method is the one ordinarily used (21; 23; 26) and under most conditions gives a fairly accurate picture, but we believe it to be slightly inferior to the technique used in the present experiment where the estimates were made on a rather fine scale so that the mean and scatter of the estimates for each value of the variable can be accurately computed.

Such a detailed analysis makes it difficult to keep the principal findings in mind so it is hoped that the following brief summary of the results will facilitate the reader's understanding of the theoretical discussion of this report:

(1) It was found that the presentation of a series of different intensity level before another series does affect the average judgment of the latter. The weaker series caused the variables of the louder series to be judged louder, which is in accordance with the principle of contrast; but the louder series caused some of the variables of the weaker series to be judged louder and some weaker. Only the latter change agrees with contrast.

(2) Fig. 5 embodies in a concise fashion most of the results discussed in sections 2 and 3 of this chapter. Reference to the figure shows that the indifference point for each series lies slightly above the middle or standard of the respective series. In this connection it will be recalled that in the case of preceding and succeeding variables we found that the variables around the average of the series were judged with the greatest amount of consistency and accuracy and these same variables were also found to reduce the variability and increase the accuracy of the judgment of any immediately succeeding variable.

(3) The figure also shows that when the effect of different standards is considered as in section 3, little or no error of estimation occurs for the variable at, or near, the average of any one series;

but if the same variables are the most intense variables of another series, they are over-estimated. Likewise it will be seen that if the middle variables of a series are made the weakest of another series, they are then under-estimated.

The similarity of the two curves in Fig. 5 is apparent, the only outstanding difference being that the time error is more positive in the 40 series than in the 30 series. The flattening out of the 30 series at the low end is probably an end effect, dependent on the entire range of intensities used in the whole experiment. The observer does not use the values below 20 dbs. freely, because he knows that the range of stimuli does not extend far in that direction. We might expect a similar upward flattening of the upper part of the 40 series—an effect of approaching the upper end of the entire range—and possibly the fact that the upper end of the curve of the 40 series does not dip as low as the upper end of the curve for the 30 series shows this effect.

CHAPTER IV

THEORETICAL DISCUSSION

It will be observed that our three methods of analysis have given evidence of the operation in the first instance of both contrast effect and its opposite; in the second instance, of a stabilizing effect; and in the third instance, of contrast effect.

To consider in detail the theoretical implications of these results, it will be necessary to deal first with the conflicting findings in the examination of the effect of a preceding series of different intensity level on a succeeding series. It will be remembered that at three of the four extremes of the two series, the shifts are indicative of contrast effect, while at the lower extreme of the weaker series, just the reverse is found. No really adequate explanation of this opposition is possible on the basis of the data, but it was found that in the second series these particular variables were judged more accurately; therefore practice may be involved. Another tentative suggestion is that the incongruity is attributable to the fact that the weakest tones were too near the limen to permit of a further downward shift in judgment.

The treatment of the data with a view to determining the influence of preceding upon succeeding variables has yielded results which can not be adequately interpreted either by the law of central tendency or by the principle of contrast. It is believed that when the data are analyzed in such a detailed fashion any effect anticipated on the basis of these hypotheses is dissipated. It is quite possible that this type of analysis reveals a more fundamental principle at work.

In this experiment a larger number of variables were used than in most experiments. Within the wide range of variables an excellent opportunity is afforded to study the effect of extreme variables (both loud and weak) upon weak, medium, and loud succeeding variables, as well as the effect of medium variables upon those three types of succeeding ones. It seems reasonable to suppose that making judgments in absolute units over a wide range of stimuli causes the variables to be compared not only with the objective standard as used in the method of constant stimuli, but also with the subjective standard of the method of single stimuli. Thus our experiment has, in a limited way, combined the method of constant stimuli and the method of single stimuli. It is an accepted experimental fact that

in the latter method the observer establishes an absolute, though subjective, basis for comparison (10; 29; 31) and furthermore, that he forms a very definite impression of the range of the series, with an indifference point around the middle.

As it has been found (20; 22; 33) that the comparison stimulus, even in the method of constant stimuli, is often compared not with the standard, but with an absolute impression, it seems feasible to suggest that the same occurred in the present experiment. Consequently, if a variable is preceded by another one which is the same as the standard, then the absolute impression of the standard is more likely to approximate the objective standard than if some other variables, quite different from the standard, preceded. Therefore this succeeding variable should be judged more accurately than if the preceding variable had been some distance removed from the standard, since this would have caused greater deviation of the subjective standard from the objective one. The direction of this deviation will vary in a plus or minus direction depending upon the particular preceding variable. Assume that, in a series with 30 as the standard, the variable 22 is preceded in one case by the variable 30 and in another case by 20. In the first case we have this sequence of stimuli:

30 : 30 . . . . 30 : 22

and the sequence of three 30's brings the subjective standard especially close to the objective, so that the variable 22 is judged on the basis of an accurate and stable subjective standard. In the second supposed case we have the sequence:

30 : 20 . . . . 30 : 22

Here the variable 20 tends to pull the subjective standard down and make it more uncertain, probably affecting the estimate of 22 both in constant error and in variability.

It is also to be expected that when the succeeding variable is the same as the standard the effect of any preceding variable will be small, since the subjective similarity of the two tones is reinforced not only because of their objective identity but also because both conform to the absolute impression of the standard of the series. It follows again that variability will be small and accuracy great. As the variables become less similar to the standard one would likewise expect preceding variables to have a more disturbing effect on succeeding ones, thereby causing more error of estimation as well as an increase in variability.

All of the above has been found in our analysis and consequently we feel justified in stating that the region around the standard does have a stabilizing effect both on the judgment of the variable which is the same as the standard and the judgment of the variable succeeding a pair in which the variable is the same as the standard. In brief, the variable equal to the standard is not only judged accurately, but also increases the accuracy of the judgment for the succeeding variable.

It should be emphasized, however, that the above finding presents no fundamental conflict with the principle of contrast; it merely demonstrates that the effect of contrast can be statistically eliminated by considering the judgment for each variable when preceded by every other variable; but when these judgments are averaged so that we have the average judgment for each variable, contrast appears. This brings us to our third analysis: the relationship between standard and variable. It may be shown that the results here can be most adequately explained in terms of contrast effect. As pointed out above it was found that the judgments extended beyond the objective limits of the series. In no case does the series narrow down or assimilate towards the mean (15). These findings are in accordance with the contrast principle, but are at variance with the law of central tendency.

The law of central tendency can be divided into rather separate and distinct parts: the first deals with judgments at the extremes of a series, stating that the smaller variables are over-estimated, the larger, under-estimated; the second part pertains to the middle of the series, where, according to it, there should be relatively little error of estimation. The results presented above agree with the latter part of the law, but are the reverse of the first part. However, it is believed that this conflict is due to the methods of interpretation rather than to actual differences in results.

It will be recalled that Hollingworth formulated the law of central tendency on the basis of results obtained from the study of reproduction of movement. The observer made a movement of a certain length, and then he attempted to reproduce his movement. The smaller distances of any one series were found to be over-estimated and the longer distances to be under-estimated. It appears that a definite statement of what is over- or under-estimated may help to clarify the issue.

For purposes of illustration it is assumed that the processes involved in reproducing a line and judging the length of a produced

line are the same. With a set-up so that distances of various lengths can be produced and reproduced the shortest length of any one series, say 20 cm., is produced for O. After a short interval this same movement is again produced for O. He is to judge the length of the second movement in regard to the first. It is assumed that he judged the second one to be 18 cms. The *second* length was under-estimated.

Now if O should instead be asked to reproduce a line of 18 cms., according to the above assumption (providing no change in attitude, set, etc., occurs in the meantime), he will make one of 20 cms. In this case there is an error of over-estimation of the *first* line.

Of the above two examples, the second is representative of Hollingworth's approach to the problem and the first is representative of the approach of the present investigator. Our next step is to consider what the over- or under-estimation refers to in each case. In the first instance the objective length of the second line is judged less than it really is; i.e., the line from an objective viewpoint is under-estimated; but if we consider the judgment from a subjective point of view, the O may be said to have over-estimated the standard. In Hollingworth's terminology, the second case would have been called an example of over-estimation. In this case, however, it is the subjective impression that is an over-estimation; the objective standard is under-estimated. Therefore, the results are actually shown to be the same.

In other words, whereas Hollingworth speaks of the judgments of the shortest (or weakest) variables of the series as over-estimations, in the present experiment these same judgments would be called under-estimations, since Hollingworth is referring to the subjective impression of the standard, while the present investigator is referring to the subjective impression of the variable.

If the law of central tendency is interpreted in the light of the above analysis our results no longer conflict with the law. Consequently both the law of central tendency and the principle of contrast require that when a tone (the standard in this case) is presented before a weak tone (e.g., the extreme variable of the series) the variable should be under-estimated to a greater degree than when the same variable is preceded by a standard which is of the same intensity. Similarly, if the standard preceded a loud tone (e.g., the extreme variable of the series) then the variable should be over-estimated more than if it had been preceded by a standard of approximately the same intensity as itself. Finally, the two hypotheses re-

quire that when a tone is preceded by another tone (the standard) of about the same intensity, the latter should have little or no effect on the former and the error of estimation would therefore be small. Table 10 shows that our data conform to all the above requirements and consequently we feel that a contrast effect was operating between standard and variable.

The prominence of the Gestalt theory of comparison makes it necessary that an attempt be made to fit the results of this experiment to that theory. If the results can be best explained on the basis of it, then additional evidence will have been added; if, on the other hand, the results are more adequately explained by another theory, it should be shown wherein the Gestalt theory fails.

Both Köhler (20) and Lauenstein (21) have found that when the temporal separation of the standard and the variable is small (less than three seconds) the variable is under-estimated. The influence of the short interval even outweighs the effect produced by the background. This is shown by the fact that Lauenstein found under-estimation for short intervals (two seconds in one case and five in another) in spite of the fact that the background was soft, which according to his principle of assimilation should yield over-estimation. It is interesting to note in this connection that when the 15,840 judgments obtained in the present experiment are thrown together, regardless of series, conditions, etc., and averaged, a positive time error of .088 is found. Since the interval between the standard and the variable was always constant (one-half second) we are justified in combining the data and comparing them with those obtained by the Gestalt school. The fact that the above figure is positive (although extremely small) does lend support to their theory,[3] but the way this figure was obtained illustrates that the direction of the error of estimation is more strongly influenced by other factors than by the temporal separation. For example, if the loudness of the series is considered we find under-estimation for the loud series and over-estimation for the weak series.

The exponents of the Gestalt theory recognize the importance of factors other than the temporal separation when longer intervals are involved; consequently it would be well to attempt an explanation of our results in terms of assimilation, regardless of the fact that they do not find the influence of this factor with an interval such as was used in the present experiment.

[3] However this particular finding was *not* in accord with their general theory, and called for a supplementary hypothesis.

To do this systematically we must consider each of our three types of analysis (page 26). The first case is where the preceding series is of a different intensity level. To apply the hypothesis of assimilation it is necessary to assume that the series presented first in an experimental sitting builds up a certain "level of reference" and that the standard of the second series assimilates towards this level. For example, if in a sitting the 40 series follows the 30 series, then there will be assimilation towards the level established by the 30 series. There will consequently be a drifting of the standard towards a lower level, and this, in turn, will cause tones to be judged louder. But if the 30 series follows the 40, the drift will be in the opposite direction and tones will, as a result, be judged weaker. It is obvious from the above that assimilation demands the same change, although for a different reason, as contrast. Thus the principle of assimilation explains the results, in this case, as adequately as contrast.

The second analysis is concerned with the influence from variable to variable. Applying the doctrine of assimilation we can hypothetically suggest that when tones are presented in pairs, the standard of the second pair assimilates towards the variable of the first pair, thereby causing a change in the intensity of the effective or subjective standard. Consequently, when the next variable is compared with this "changed" standard, the judgment rendered will be affected by this change. If this be true, then one can assume that when the standard (for example 30) succeeds a variable such as 20, the subjective standard will drift in the direction of 20 and thereby suffer a reduction in intensity. Thus the next variable will not actually be compared with 30, but rather with a weaker tone (the absolute intensity dependent upon how much assimilation has taken place). From this it follows that the error should be one of over-estimation, but the reverse was found. Likewise if the preceding variable is 40 and the standard 30, assimilation will tend to make the next variable be compared not with 30, but with a tone slightly louder than 30. Thus there should be more under-estimation at 40 than at 30 (or at 50 than at 40 in the 40 series). The last graph of both Figs. 1 and 2 shows this to be the case. If assimilation is to hold throughout, however, the lines in Figs. 1 and 2 should become increasingly positive as one goes from the weakest to the loudest variable of a series. Although these lines are not perfectly smooth, they do seem to slope in the direction required by the principle of assimilation. Thus we see that assimilation in this instance de-

mands that the weaker preceding variables should cause all stronger succeeding variables to be over-estimated and the louder preceding variables should cause all weaker succeeding variables to be under-estimated. Or if under-estimation occurs for all variables, then there should be less under-estimation when the preceding variable is weak. This is essentially what contrast requires, and as it has already been shown that neither contrast nor central tendency alone offers a satisfactory explanation, we can conclude that the hypothesis of assimilation is no more adequate than the other two principles.

The third analysis investigates the influence of the standard upon the variable. The writer is aware that the advisability of applying the theory in this case is questionable. It must be assumed that the trace left by the standard assimilates toward the "stillness-trace." The logical places for this trace to assimilate are toward a zero intensity level, or possibly toward the level to which we are most accustomed. The first case should cause all variables to be over-estimated; but this was not found. The second case is, at present, too hypothetical to deal with; we have no way of determining to what customary level the assimilation will approach. The range of intensity used in this investigation (20–50 dbs.) can easily be said to be the range to which we are usually exposed. But we find a very definite change from under- to over-estimation as we go from low to high intensities of a series. Consequently, the writer feels that the psychological explanation, involving contrast, is more appropriate than the Gestalt explanation.

It is suggested (36) that the results of this experiment might be made more intelligible by conceiving of the absolute impression of the series as a mental ruler and by comparing this mental scale with the objective scale of the series. It is assumed that an absolute impression of the series is established during the early part of the experiment. This impression can be thought of as a mental ruler with mental divisions occurring on both sides of the reference point, which is assumed to be the average of the series. Since this mental scale is based upon the stimuli presented, it necessarily follows that it is influenced and changed by stimuli.

The mental ruler may differ in four ways from the objective scale:

1. It may be displaced bodily upward or downward along the objective scale; for example, it might be displaced 1 db. downward, so that the subjective 20 db. was objectively 19 db., 30 db. objectively 29 db., 40 db. objectively 39 db., and so for every step on the subjective scale.

2. It may be contracted (or expanded), so that while 30 db. subjective corresponded exactly to 30 db. objective, 20 subjective corresponded to 22 objective, 40 subjective to 38 objective.

3. It may be, and almost certainly will be, less stable than the objective, so that subjective 20 db. might at one moment coincide with an objective 18 db., at another moment with an objective 20 db., and at a third moment with an objective 22 db.

4. Finally, whereas the divisions on the objective scale occur at regular and equal intervals, those on the mental ruler may be unequal when viewed objectively. Thus each division on the subjective scale is supposed to represent, for example, two decibels, with 30 dbs. being the mean of the series (or the point of reference). Now the next division on the subjective scale also represents two dbs. (or 28 dbs.), but it corresponds objectively to say, 28.1; the next division represents 26, but corresponds objectively to 25.8; 20 on the subjective scale corresponds to 21.3 on the objective scale; finally, 16 on the subjective scale corresponds to 21.1 on the objective scale. Of course bodily displacement of the whole subjective scale might be combined with contraction, as well as with instability. Also the instability might differ in amount at different parts of the subjective scale. With these possibilities in mind we shall now attempt to show how the above results can be stated in terms of them.

The first case to be considered concerns the effect of a series of different intensity level on another series. We found that the preceding series did affect the succeeding series, but the effect was not always one of contrast. Since we have assumed that the mental scale representative of the absolute impression of the series is quickly established and furthermore that it is very sensitive to stimuli, we would expect the preceding series to have very little effect on the succeeding series. Such a conception would permit of an explanation for the finding that the variables of the weak series are judged just as accurately when a loud series preceded. But to explain the effect of the weak series upon the loud it would have to be assumed that the subjective impression of the loud series was not only influenced by the preceding weak series, but also that this influence lasted longer than we postulated above. Accepting this we could then say that the subjective was lower than the objective scale and consequently all tones of the loud series were judged louder when they followed the weak series. That the above is not entirely satisfactory is obvious.

In our analysis of the influence of variable upon variable we

found the variables around the average of the series to be more consistently and accurately estimated than any others. We also found that these variables, when considered as preceding variables, increased the accuracy and consistency of the succeeding judgment.

In an attempt to test the possibilities listed above we decided to analyze the data by a rather backhanded method. That is, the responses were collected into classes according to the response (instead of stimulus). We are assuming that when any variable is judged, say 20, the momentary subjective location of the 20 db. point on the subjective scale is revealed. Thus all the judgments "20" are grouped together and the mean objective value for this response is calculated. The mean objective value for each two db. group of responses, along with the standard deviations are presented in Table 12. This table also includes the difference between each two consecutive averages. In Table 13 the distribution of the responses in terms of percent will be found. For example, in the 30 series, 64% of the responses below 15 was given when the stimulus was objectively equal to 20 dbs.; 25% of these responses was given when the stimulus was 22 dbs., and so on.

TABLE 12

THE FOUR OBSERVERS' RESPONSES AND THEIR OBJECTIVE EQUIVALENTS

30 series presented first in an experimental sitting

Subjective response	*N*	*Objective equivalent*		
		Average	*Diff. between averages*	*S. D.*
Below 15	36	21.06		1.68
15–16	98	21.18	.12	1.73
17–18	88	21.36	.18	1.66
19–20	400	22.54	1.18	2.31
21–22	224	23.31	.77	2.42
23–24	248	24.11	.80	2.76
25–26	345	25.86	1.75	3.11
27–28	260	28.11	2.25	2.67
29–30	765	30.46	2.35	3.10
31–32	167	32.55	2.09	2.71
33–34	158	33.70	1.15	3.02
35–36	241	35.16	1.46	2.74
37–38	190	36.04	.88	2.42
39–40	346	37.41	1.37	2.27
41–42	98	38.37	.96	1.37
43–44	115	38.73	.36	2.02
45–46	80	39.18	.45	1.04
47–48	36	39.72	.54	.81
49–50	62	39.00	–.72	1.56
Above 50	3	38.67	–.33	1.82
	3960			

40 series presented first in an experimental sitting				
Subjective response	N	Objective equivalent		
		Average	Diff. between averages	S. D.
Below 19	13	30.62		1.09
19–20	62	30.94	.32	1.22
21–22	30	31.20	.26	1.51
23–24	50	31.36	.16	1.62
25–26	119	31.78	.42	2.03
27–28	103	32.19	.41	2.21
29–30	466	32.92	.73	2.56
31–32	188	33.88	.96	2.38
33–34	194	34.98	1.10	2.37
35–36	305	36.31	1.33	3.03
37–38	258	38.52	2.21	2.73
39–40	708	40.90	2.38	2.92
41–42	163	42.64	1.74	2.61
43–44	156	44.35	1.71	2.17
45–46	268	45.49	1.14	2.16
47–48	211	46.64	1.15	2.02
49–50	356	47.52	.88	2.64
51–52	103	48.83	1.31	1.49
53–54	85	49.29	.46	1.34
55–56	65	49.11	–.18	1.41
Above 56	57	48.88	–.23	1.64
	3960			

An inspection of Table 12 shows that the objective and subjective scales agree most closely around the mid-point of the series. Therefore it seems logical to suppose that variables around the average of the series should be judged more accurately and with less variation than any other variables. It is conceived that the divisions on the mental scale around this region are more stable and consequently they would not be easily affected by preceding stimuli.

When one of the center variables is judged the mental scale benefits by it, since such judgments tend to bring the subjective scale into closer agreement with the objective. Thus the next variable is judged under more favorable conditions and consequently the estimation will be a closer approximation of the objective value of the stimulus than if the preceding variable had been an extremely weak or loud one.

Finally, in considering the effect of the standard upon the variables, with the present analogy of a mental ruler in mind, the following relationships are revealed. Here, as before, the analysis is limited to those stimuli which were present in both the 30 and the 40 series.

In the graphs below, the top line in each is used to represent, schematically, the objective scale and the lower line in each represents the subjective scale. It will be noted that every division on the objective scale is equal to one decibel.

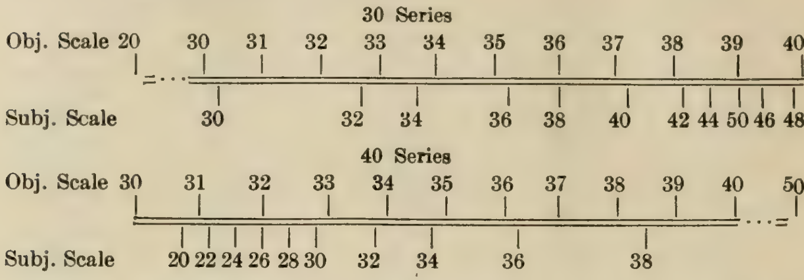


Fig. 6. Comparison of objective and subjective scales of the 30 and 40 series when presented first in an experimental sitting.

On the subjective scale, however, only the divisions around the average of each series approach a one-to-one relationship with the objective divisions. It was pointed out above (page 47) that the judgments extended below the limits of the objective scale, and it follows,

TABLE 13

The Distribution of the Responses Given by Four Observers

30 series presented first in an experimental sitting

Response	*Percent of subjective responses*										
	Objective equivalent										
	20	22	24	26	28	30	32	34	36	38	40
Below 15	64	25	8	0	3	0	0	0	0	0	0
15–16	60	24	11	4	0	0	0	0	0	0	0
17–18	48	42	5	6	0	0	0	0	0	0	0
19–20	31	32	24	10	3	1	1	0	0	0	0
21–22	21	23	30	19	6	0	0	0	0	0	0
23–24	15	21	27	25	10	1	1	1	0	0	0
25–26	6	13	19	28	21	10	2	1	1	0	0
27–28	1	2	11	16	31	28	9	2	0	0	0
29–30	1	1	2	8	18	25	26	13	5	1	0
31–32	0	0	1	2	7	17	28	26	15	4	0
33–34	1	1	1	1	2	9	18	40	18	8	3
35–36	0	0	0	1	1	4	11	28	31	19	6
37–38	0	0	0	0	1	2	7	22	34	23	12
39–40	0	0	0	0	0	1	3	8	27	32	28
41–42	0	0	0	0	0	0	0	1	15	48	36
43–44	0	1	0	0	0	0	0	0	8	40	51
45–46	0	0	0	0	0	0	0	0	6	29	65
47–48	0	0	0	0	0	0	0	0	0	14	86
49–50	0	0	0	0	0	0	0	3	8	24	65
Above 50	0	0	0	0	0	0	0	0	33	0	67

40 series presented first in an experimental sitting

Response	*Percent of subjective responses*										
	Objective equivalent										
	30	32	34	36	38	40	42	44	46	48	50
Below 19	77	15	8	0	0	0	0	0	0	0	0
19–20	63	27	10	0	0	0	0	0	0	0	0
21–22	53	37	7	3	0	0	0	0	0	0	0
23–24	50	36	10	4	0	0	0	0	0	0	0
25–26	47	26	18	9	0	0	0	0	0	0	0
27–28	35	36	17	10	3	0	0	0	0	0	0
29–30	26	30	23	15	5	0	0	0	0	0	0
31–32	12	22	36	22	6	1	1	0	0	0	0
33–34	6	14	26	35	16	3	0	0	0	0	0
35–36	5	10	17	26	27	11	5	1	0	0	0
37–38	1	2	7	14	34	29	11	2	0	0	0
39–40	0	1	2	5	15	28	28	15	5	1	1
41–42	0	0	0	1	9	17	27	27	17	2	1
43–44	0	0	0	0	0	6	19	39	24	8	3
45–46	0	0	0	0	0	2	9	30	35	16	7
47–48	0	0	0	0	0	0	3	14	41	31	11
49–50	0	0	0	1	1	2	3	8	15	39	32
51–52	0	0	0	0	0	0	0	1	7	42	51
53–54	0	0	0	0	0	0	0	0	6	24	71
55–56	0	0	0	0	0	0	0	3	6	23	68
Above 56	0	0	0	0	0	0	0	4	14	18	65

therefore, that there must be a concomitant decrease in the number of decibels between each two consecutive divisions on the subjective scale when these are stated in terms of their objective equivalents. The data show that this contraction occurs only at the extremes of a series.

In the data, these phenomena show up as an over-estimation of the stronger variables of a series, and an under-estimation of the weaker variables of a series. For example, a subjective response of 29–30 in the 30 series corresponds objectively to 30.46, while the same response in the 40 series corresponds objectively to 32.92; a subjective 33–34 in the 30 series equals an objective 33.70, while in the 40 series, it equals 34.98; a subjective response of 39–40 in the 30 series corresponds to an objective 37.41, while in the 40 series, it corresponds to an objective 40.90.

It is hoped that Figure 6 will demonstrate more clearly the difference between the 30 and 40 series, in respect to this relationship of the objective and the subjective scales. It seems evident from these graphs that judgments of stimuli which are close to the standard and therefore reinforced by it, more closely approximate the objec-

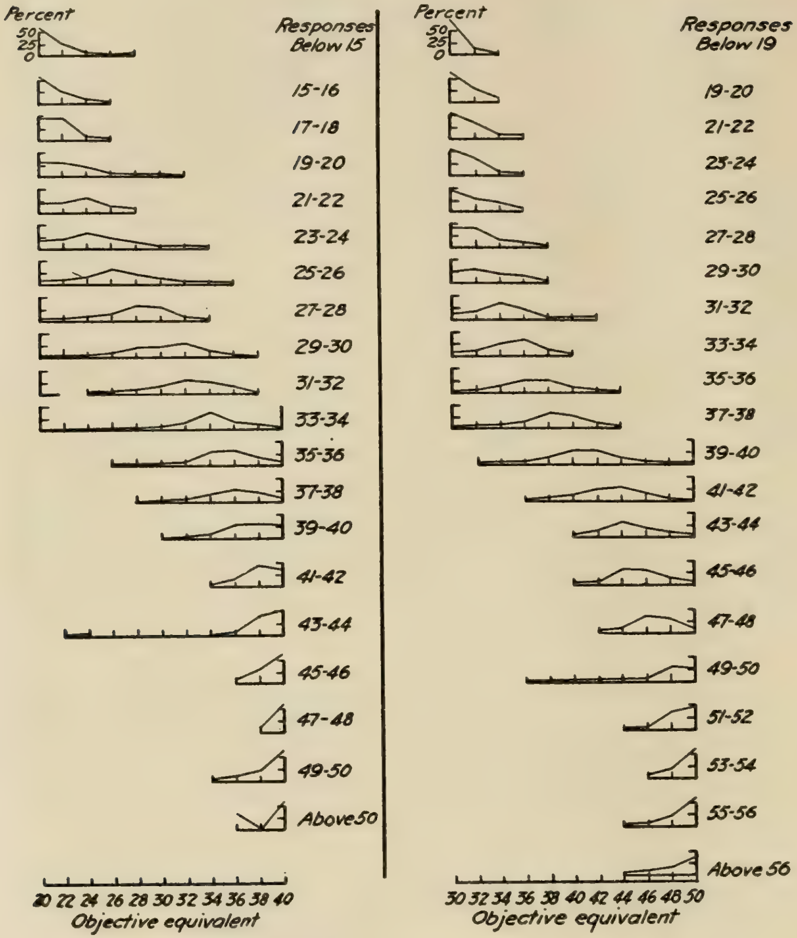


FIG. 7. The distribution of the responses given by four observers. (Data from Table 13)

30 series first — 40 series first

tive value of the stimulus than do judgments of these same stimuli when they are the extreme variables of a series.

The distribution in Table 13 seems, however, to indicate the operation of an end error. It is likely, for example, that responses of below 15 would have been elicited by stimuli below 20, if there had been any, and the same is clear for responses 19–20, 21–22, 23–24, and even 25–26. Similarly at the upper limit, responses of 37–38 would have been given to stimuli above 40 had there been any such in the series. The same is true for both ends of the 40 series.

The excess of responses at the subjective scale value 20 may very well be due to the absence of any stimuli below 20, which if present would have been judged 20. Consequently, the true mean for this response would be smaller than the obtained mean. If the end error could be removed we should have the subjective 20 somewhere near the objective; and similarly at each end of both series.

A method by which the end error can be eliminated is not obvious to the writer at present. Any work involving the use of series makes this effect inevitable. In psychophysical experiments, therefore, the error is always present but since the judgments are usually made in two or three categories, no indication of the error is obtained. The technique of recording judgments in absolute units brings this error out in an obvious fashion, but since it operates in both methods, the method of absolute units can not be said to be inferior on this score. In the present experiment some attempt was made to eliminate the importance of the error by using a large number of variables in each series. However, Table 13 shows that this was not altogether successful since only 5 or 6 of the variables of each series appear to be free of the error. It might be possible to further reduce the error by using an even larger number of variables and, after an analysis similar to the one presented in Table 13, discarding those classes of responses that do not yield a fairly normal distribution of objective equivalents. The remaining data could then be analyzed as in Chapter III. This would afford an opportunity for determining just what part the end error plays in such data.

CHAPTER V

SUMMARY AND CONCLUSIONS

This study was concerned with the effect of preceding stimuli upon the judgments of succeeding stimuli. The experimental situation was arranged so that the effect of three types of preceding stimulations could be studied.

The stimuli consisted of auditory intensities varied, by means of an attenuator, over the range of 20 to 50 decibels in steps of two decibels. The frequency of the tone (1000 cycles per second) remained constant throughout the experiment.

The method of constant stimuli was used. The observers, however, judged the variables in absolute units. Such procedure allowed ordinary statistical techniques to be employed. For example, the mean and standard deviation of any group of judgments could be calculated. Consequently, a detailed analysis of the data was possible.

By using two series of different intensities and by having them follow one another in the same experimental sitting it was possible to study the effect of the loud series upon the weak, and vice versa.

Since the prearranged order of the presentation of the variables was such that each variable preceded and followed every other variable an equal number of times it was possible to study the effect of preceding variables upon succeeding variables.

As the variables of the two series overlapped it was possible to study the effect of different standards upon the same variable.

Considering the influence of one series upon another series of a different intensity level:

1. The variables of the loud series are judged louder when the weak series preceded than when the former is presented first in an experimental sitting. This shift was found to be statistically reliable when the series as a whole is considered.

2. An analysis of the judgments for each variable of the loud series indicates that the preceding weak series caused a shift in the loud direction for the average judgment of each variable (except in one case). Such shifts indicate the presence of a contrast.

3. The loud series caused the judgments of the weak series also to be judged louder (although the difference is not statistically reliable). This is the reverse of what a contrast effect would provoke.

4. When the judgments for each variable of the weak series are

considered separately conflicting results occur. The weaker variables of this series are judged louder when the loud series preceded the weak; but the louder variables of the weak series under the same condition are judged weaker. The results for the weaker variables of the weak series make it impossible to conclude that a contrast effect is operating between the preceding and succeeding series.

In our analysis of the effect of the preceding upon the succeeding variable we found evidence of a stabilizing effect.

1. This effect is produced by variables near the average of the series.

2. The judgments of such variables are more accurate and more consistent than those of any other variables. These variables are least affected by preceding variables.

3. The middle variables, when considered as preceding variables, increase the accuracy and consistency of the judgments of the succeeding variables.

4. This stabilizing effect can be explained neither by the law of central tendency nor by the principle of contrast.

Our analysis of the effect of different standards upon the same variables permits of the conclusion that there is a contrast effect operating between standard and variable.

1. The weaker variables of the series are under-estimated.

2. The louder variables of the series are over-estimated.

3. When either the weaker or the louder variables are made the middle variables of a series there is practically no error of estimation.

An attempt was made to reinterpret the law of central tendency. If this exposition is correct then there is no fundamental conflict between the law of central tendency and the principle of contrast.

This experiment has demonstrated that under certain conditions, stimuli oppose each other in such a way that a weak stimulus preceded by a strong one is judged weaker than it actually is, and vice versa. This is referred to as contrast and its presence has been found in experiments employing a variety of stimuli: namely, tones (23; 26) and weights (9; 30) in the usual psychophysical experiments, and colors, tones, and odors in experiments on hedonic tone (3; 4; 5). Thus the reality of the phenomenon can not be doubted, but an explanation of *why* contrast operates or what processes (either psychological or physiological) underlie it, must be postponed until the conditions under which it occurs, or does not occur, are better known.

The writer is inclined to prefer the purely psychological explanation, in spite of the fact that it might be argued that the principle of contrast is not an explanatory, but at most, a descriptive concept. Any attempt to postulate a physiological basis would, at present, be unjustifiable; furthermore such an hypothesis could neither be proved nor disproved. Consequently, we feel that it is at least justifiable to state that some conditions have been determined under which contrast operates, while a statistical technique for ruling out the effect of contrast has also been devised.

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